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 SECTION OF TROPICAL DISEASES
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DISCUSSION ON THE PROPHYLAXIS OF
 MALARIA

J.W.W. Stephens et al.

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PROCEEDINGS OF SECTIONS.

SECTION OF TROPICAL DISEASES.

Brig.-Surg.-Lieut.-Col. ALEXANDER CROMBIE, I.M.S. (Ret.),
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DISCUSSION ON THE PROPHYLAXIS OF MALARIA.

I.—J. W. W. STEPHENS, B.A., M.D. Camb.,

Walter Myers Lecturer in Tropical Medicine, Liverpool University.

I THINK it may help to make our views more precise if, instead of recounting the methods of prophylaxis used and the results achieved in Italy, in British, French, and German tropical possessions, in Japan, I bring before you some scientific aspects of the problem which in many of the publications I have read on this subject seem to me to be overlooked. I will assume in this paper that I am dealing with the occurrence of malaria in our tropical possessions. It is, I believe, not unnecessary even now to point out how malaria is contracted. It has been for centuries believed that malaria is contracted from the vapour of marshes; and now that certain mosquitos are proved to be the cause, it is not uncommon to overlook the means whereby the mosquito produces malaria, and still to attribute malaria to the presence of the water in which the mosquito breeds, the "shallow pools," etc. What seems to me to be necessary is that the public should be educated to regard not the pool, not the mosquito, not even the Anopheline, as the source of danger, but the *infected* Anopheline; and I would go further: I would have everybody clearly understand how it is that the Anopheline becomes infected—that is, can convey malaria. It results from the fact—which, I believe, not in too laymen in the tropics knows—that malaria is a contagious disease. The source of contagion, unsuspected by the layman, lies in the fact that in the tropics the native population, especially the child population, carries malarial parasites in its blood; that it does so often while presenting not the least outward sign of sickness. It is this apparently healthy but actually highly infected population which is the great source of malaria parasites, and hence of danger, to the European. It is from the native that the malaria-bearing mosquito gets its parasites, and these it transmits again after the lapse of a certain time to the European. I have said that the native population may show no outward sign of sickness. I do not forget, however, that the native also dies from malaria, but if we wish to see malaria in its severest forms, let us visit some up-country miner's camp or European settlement in the bush. There we shall find those "malarial wrecks," the fever-stricken pioneers, "down with fever," as it is called, vomiting incessantly, with a temperature of 105°, in constant dread of black-water fever, which not uncommonly supervenes. Those who live in the towns not uncommonly forget that it is in these out-stations that malaria exists in its most virulent form. I have dwelt on this condition because it is the lot of these unfortunate Europeans to fill the "white man's grave," because I think that in schemes for eradicating malaria they are forgotten, because I think that many of these lives can be saved by means which I will specify later, and that we can legitimately do so even at the expense of delaying that general eradication of malaria from the native which will be the work of years, and which will not be achieved in our time.

Malaria is indeed a contagious disease. It has been the great discovery of Ross that it is a certain mosquito that conveys the contagion. Let me review briefly the state of our knowledge on this point. We know now that certain mosquitos of the sub-family *Anophelina*, alone can convey the contagion. Now of Anophelines there exist about a hundred species. Which of these, or do all of them in different parts of the world convey malaria? It is surprising that on this point we have but scanty information.

The number of species that we know can convey malaria we may almost count on our ten fingers. They are in Europe

—*A. maculipennis* (mainly); *A. bifurcatus* (less concerned); *P. superpictus*; *M. pseudopictus* (especially in some parts). In Africa *A. maculipennis* (in Algeria); *P. costalis* and *M. funesta* (especially in tropical Africa), and *P. Chaudoyei*. In India *M. listoni*, *M. culicifacies*, *N. maculipalpis* (?). In North America *A. maculipennis*. In West Indies *Ce. albipes*. That is, a dozen species only of which we can say with any certainty they convey malaria out of nearly a hundred existing species. But it may be urged, if one species conveys malaria another will; that is not a scientific argument, and nothing short of actual proof should be allowed to satisfy us. We can go further, and say that we have a certain amount of evidence, gradually increasing, to show that all species do not convey malaria.

Let me recall to your mind the results obtained by Christophers and myself in India. We had formed the hypothesis, based upon general observations, that all species of Anophelines were not concerned in the transmission of malaria. We put this hypothesis to the test in two very malarial places in India—Mian Mir (Punjab) and Ennur (Madras). We caught in the same huts, at the same time, in these two far distant places two species of Anophelines—*M. rossii* and *M. culicifacies*. We determined by actual dissection which of these species contained parasites (sporozoites), and found as follows:

	Number Dissected.	Number Infected.	Percentage.
In Mian Mir:			
<i>M. culicifacies</i> ..	259	12	4.6
<i>M. rossii</i> ...	496	0	0
In Ennur:			
<i>M. culicifacies</i> ...	69	6	8.6
<i>M. rossii</i> ...	364	0	0

So that *M. rossii* was not carrying malaria under conditions where *M. culicifacies* was. These observations have been repeated by others and the result is always the same. *M. rossii* does not in nature convey malaria. This, while scientifically of great interest, is also of practical value. Shall we spend money and occupy our time in eradicating *M. rossii* when as far as we know it is not concerned in the transmission of malaria? These mosquitos exist in myriads in Calcutta, Madras, Bombay, and in all large towns. We believe them to be harmless.

But this is not an isolated instance. Recently Hirshberg in the United States has got similar results, though in this case by feeding experiments. Two species—*A. maculipennis* and *A. punctipennis*—were fed on the same case of malignant tertian malaria under the same conditions, with the following result:

	No. Fed.	No. Infected.
<i>A. maculipennis</i> ...	45	8
<i>A. punctipennis</i> ...	58	0

Whether *A. punctipennis* conveys malaria in nature remains to be seen. Similar results (unpublished) have been obtained in Japan. These results, then, show that certain species of Anophelines do not convey malaria, and on general grounds there is little evidence to connect those species which we know as "wild" mosquitos with the transmission of malaria. Anyhow, it should not suffice to state simply "the *Anopheles* was found" without stating what species of Anopheline; but, most important of all, it should be determined by actual dissection whether or no that species does transmit malaria.

The practical bearing of these facts is obvious. If we can positively state that a certain species of Anopheline does not transmit malaria, it will obviously simplify operations directed against mosquitos. Another point of great interest in connexion with Anophelines is their breeding grounds. Take again the two Indian species mentioned above, *M. rossii* and *M. culicifacies*. Christophers and myself showed in India that, broadly speaking, each Anopheline has a breeding ground of its own. At any rate, in the case of the above two species they are totally distinct and different in nature, and whereas *M. rossii* breeds in the shallow pool or puddle some inches deep only, *M. culicifacies* does nothing of the kind. It is never found breeding in these dirty puddles; it loves clear, fresh water, of streams, rivers, canals, and other moving water, where it occurs in myriads. Now if our results are true, that *M. rossii* does not carry malaria, and no doubt has yet been thrown on them, it means that it is unnecessary to

attack the breeding grounds of *M. rossii*. This means an immense saving in labour and expense, for even during the dry season the pools in which *M. rossii* breeds are very numerous, and during the rains they are legion. Minute work, then, is still required on breeding grounds, on the habits of larvae, their enemies—fish, beetle, beetle larvae, and the like, natural larvicides so to speak—all these points may be of great practical import.

Then, again, there is the question of the flight of mosquitos, a very important problem. Christophers and myself were constantly engaged in trying to make observations on this point, because we used to read alarming statements published by hasty writers that mosquitos (genus and species it is needless to say not mentioned) flew fifteen miles, so that it seemed to us, if this were so, whatever might be done to free a place from Anophelines, there was nothing to prevent some of these long fliers coming, with parasites in them, into a district just made free and undoing all one's labours. But close observations convinced us that nothing of the kind happened, or perhaps, to be strictly accurate, did not happen under the conditions we knew of, and all our observations, repeatedly made and checked, tended to show that the normal flight of Anophelines was a matter of some hundred yards and not miles. Thus we observed that the centre of Freetown, the capital of Sierra Leone, was completely free from Anophelines. We slept during many months there at all seasons of the year in four different houses and never did we see Anophelines in our rooms—*Stegomyia*, of course, in hordes. But a quarter to half-a-mile away it was very different; there Anophelines were as thick as bees. What was the difference? The centre of Freetown was in fact a town with large stone houses, open streets, and moderately well-arranged surface drains. But in the outskirts we come to the streams, with their countless larvae, in the back-waters and pools, and to the native huts, infested with Anophelines, having therein a plentiful supply of blood. Why, then, should they fly abroad and visit the centre of the town? In fact they did not, and the result is this important condition, that in the centre of Freetown, a quarter to half a mile from deadly centres of infection, you have a segregated protected area, where the danger of contracting malaria is extremely remote.

Again, by a series of observations we made in India we were able indirectly to come to somewhat similar conclusions. We sought to determine why, of a series of villages in the same district, one was highly malarious, another only slightly so, and we found that this was almost entirely dependent on the number of Anophelines there present, and that the number of Anophelines was dependent on another factor—the presence or absence of breeding grounds past or present; so that when a village was distant three-quarters of a mile from breeding grounds the amount of malaria—the endemic index as we termed it—was reduced to very low dimensions—5 per cent. or 0 per cent., instead of 50 to 70 per cent. where breeding grounds surrounded the native huts. We concluded from these observations that a distance of about a mile from breeding grounds was sufficient to protect that village from malaria, consequently that here at all events this distance was beyond the normal flight of Anophelines. The distance in Freetown was decidedly less than this, a quarter to half a mile. These observations seemed to us of some importance as indicating the distance that sufficed to give protection from mosquitos, or at least a considerable if not an absolute degree of protection. This question of flight of course requires still further study, since, as we shall see later, under certain conditions it may be a complicated one, resulting in distances as great as one to two miles being traversed. We see, then, that various problems still exist for a more complete solution than we were able to give in the time at our disposal, and that these problems have considerable bearing on the question of prophylaxis, and that the prophylaxis of malaria will not be best achieved by blindly proceeding to act against mosquitos as a whole, but by careful action based upon accurate study of all the habits of mosquitos. I have unduly prolonged these preliminary considerations, but their importance is in my opinion immense, and they are at present practically completely neglected.

In the remaining portion of my paper I will consider very briefly modes of prophylaxis, pointing out what in my opinion are the merits or demerits of each system. For in fact it would appear from the advocates of these systems that one and only one method exists—that is, the one advocated by the particular person. Thus it seems to be the aim of the advocates of each system to decry that of others. This does not appear to me to be the best solution of the problem, for

the method that may succeed in one place may quite conceivably fail in another where the conditions are entirely different.

Now the measures usually adopted are:

1. *Antilarval*.—Of this method I would say that it appears to me to be fundamental and thorough, provided it takes the form of systematic drainage, and that it is the method which should as a sanitary reform be followed as a steady continuous policy.

Its merits consist in the obvious fact that as the mosquito breeds in water, if this water can be drained away, the number of larvae in that area must diminish, and probably, if there is no influx from without, the number of mosquitos, and also of malaria. It seems to me that this method should take from the first this form—namely, drainage—and that it is not wise to substitute for it petrolage, except as a temporary makeshift, without any attempt to diminish the real source of the mosquitos—namely, the water.

Let us now consider by what means it is possible to estimate the success or want of success of antilarval measures. In other words, the diminution of malaria.

I will not consider the means of estimating the number of larvae or the number of Anophelines, suffice it to say that it is quite easy for anyone who has made systematic observations on larvae and mosquitos throughout one year easily to appreciate whether there is a diminution in succeeding years, but that is not the practical question, which is, is the malaria diminished? How can we estimate this? Now I believe we have found a test more accurate and sensitive than any of those on which we had previously relied. It lies in the variation in the "endemic index" of the native population. Christophers and myself found in West Africa at the same time as Koch in the East Indies that the child population contained numerous parasites in their blood, although apparently healthy (a condition I venture to state quite unsuspected by those who were aware that the mortality of children was partly due to malaria). We found, then, that by estimating the percentage number of children at any particular age, say under 10 years, which contained parasites in their blood, it was possible to get a very accurate idea of the variations of endemic malaria. By this simple means we were able to show, for instance, that in spite of public opinion on the prevalence of malaria in the suburbs of Calcutta, malaria was not endemic in the population there, but, on the other hand, when we reached the ill-famed Terai, at the foot of the Himalayas, there indeed malaria was intense, reaching the figure of 72 per cent. in the particular district examined by us. I cannot conceive by what other means such a remarkable difference could have been shown. Anophelines existed in myriads in the suburbs of Calcutta; that was no test. (We were asked by the doubting editor of the *Indian Medical Gazette*: If not with malaria, with what, then, are our hospitals filled? The answer is now being given: With "cachexial" fever.) That was the test of hospital admissions. Hospital statistics, I need hardly point out to you, require the most careful scrutiny. To take one source of error only: Imagine the effect on statistics where a new medical officer enters nothing as malaria except those cases he microscopically diagnoses as containing parasites, where the old medical officer entered practically everything as malaria that had a temperature. Such instances have occurred. In the endemic index we have the best and surest test at our disposal. What other test so accurately and sensitively shows, for instance, the variation in endemic malaria from month to month?

Finally, I would point out the absolute necessity for control observations and the neglect of these in so many published results. I would say on this question of estimating the success of operations that two conditions are necessary to be fulfilled:

a. That the endemic index of the populations must show a decrease.

b. That the control areas must remain unchanged.

If these are fulfilled, to me the success of any operations will be convincing, even if hospital statistics showed the reverse, which I should not expect.

2. *Mechanical Prophylaxis by Wire Netting, etc.*—That by this means excellent results can be got has been shown by the Italians. The Japanese have likewise applied it to their troops, with the remarkable result that in a particular regiment the entries diminished from 2,000 odd in 1900 to 256 in the first six months of 1903. We are told that the sentries wore veils and gloves at night!

3. *Quinine Prophylaxis*.—I believe this method, too, excellent, and the success attending it remarkable. How far it is applicable in every locality local conditions must decide. I believe its applicability is somewhat limited, but this should not hinder its adoption in suitable circumstances. In many isolated villages I believe malaria could readily be stamped out in this way, where antilarval measures would be impossible owing to the fact that the larvae breed in the rice swamps surrounding the villages. A quinine prophylaxis among Europeans in small settlements has given in the hands of the Germans excellent results. The so-called gram prophylaxis, repeated on two consecutive days, has in certain instances been thoroughly effective, and it should be noted that there is no danger of the quinine producing blackwater fever, for, on the contrary, it is among those insufficiently protected with small doses—that is, among those suffering from malaria—that quinine induces blackwater fever.

4. *Segregation*.—Next, I would call your attention to a mode of prophylaxis which you may not even have heard of—namely, “segregation of Europeans.” It was, however, recommended by Ross as “one of the fundamental principles which the State must adopt in order to extirpate malaria in tropical cities.” Major Ross has informed me personally, however, that this was done on general sanitary grounds only, and not on the special grounds resulting from the demonstration by Christophers and myself that the natives were the source of European infection in the tropics. I think, then, that we may claim in our report to the Royal Society of May, 1900, to have first put forward this as a definite prophylactic measure based upon our knowledge of how Europeans are infected. Manson has also declared segregation to be the first law of hygiene for the Europeans in the tropics. We were led to advocate this measure with all the seriousness and urgency we could, because we had seen for ourselves during two years’ travelling in British possessions in Africa the terrible effects resulting from its neglect. That it was a practical policy we had no doubt. We had, in fact, seen it carried out—on general grounds—in Accra, where most of the officials enjoy the protection of well isolated bungalows well separated from the insanitary native huts; insanitary in a very special sense, for it is here that Anophelines are infected. We had seen that the segregation of Government House, Freetown, a quarter of a mile from native huts, afforded complete protection. It is a policy that can be carried out in the case of single bungalows, situated in the bush, in larger settlements, and even eventually in towns. And yet an African Governor has denounced it as academical (although actually in existence in Accra), impolitic, and so forth. That it is not so is shown by the fact that another African and unprejudiced Governor, who had no system of his own, has actually carried out this prophylactic method, and, personally, I rejoice to learn that in several Gold Coast stations the Europeans now enjoy segregated cantonments. The principle, in fact, in its widest application is that which we see over the whole of India, namely, European cantonment and native bazaar. In its more limited and still easier application it means that the European bungalows should not be built as now, in the midst of a native village, surrounded by huts, swarming with infected Anophelines, but that an isolated site some half-mile away should be chosen. I believe the immunity then given to Europeans living hitherto in the midst of infected Anophelines will be very great. It was, we felt very strongly, an urgent matter, one easily carried out, and one that would at once save many lives, while measures for draining large towns would be a matter of many years. But it is also said that it is not a just method, for the native is neglected, and the native should be treated as the white man. Within limits this is true, but it is the limits that alter the nature of the case. When the sanitary and general education of the native population has made any progress whatever, it will then, perhaps, be possible to begin to treat natives and Europeans alike; but until that happy day arrives it is, I maintain, not right to sacrifice the European lives that are daily sacrificed to such false sentimental ideas.

I have said little of actual operations and actual results in this paper, because you will have papers on this subject later. I have urged the necessity for critical inspection of these results. I will not here contrast, for instance, the success of antimalarial operations at Ismailia with the failure at Mian Mir; I will only say a word on each. At Ismailia the campaign was against mosquitoes as a whole. It is difficult to know what the effect on the Anophelines has been; no data are yet forthcoming. The operations of the mosquito brigade were very

largely devoted to treating cesspits which were the breeding grounds of *Stegomyia*, a process which, of course, could not affect malaria one way or another, and so far as it is not clear what has been done to the Anophelines. We do not even know what Anopheline is carrying malaria at Ismailia, yet we have the brilliant result of the diminution of the fever entries from an average of 2,000 to 200 roughly. Of Mian Mir I may say a word of explanation, as I myself took part in the preliminary survey of the place. Some misconception has arisen as to why Mian Mir was selected, a place now proved to be very difficult to deal with. We selected it because (1) it was highly malarious, and (2) because it had an extremely small rainfall, and we thought that antilarval measures would consequently be easy. It was only on arriving at Mian Mir that we found the whole place to be a network of irrigation canals, and even had we known this we might not at first have seen its importance, for at that time it was the prevalent opinion that Anophelines “bred in shallow puddles.” Anyhow, we could find no other place where it seemed at all likely that success would attend our efforts in the time at our disposal, since we did not know when we might have to return home.

I will say, however, of the Mian Mir experiment that I believe it stands alone in many respects. In no place that I know of has the preliminary survey of a place been carried out with the thoroughness with which it was done there. All the species of Anophelines were collected. It was determined which were carrying malaria. The breeding places of each species were carefully mapped out. The endemic index of the various native bazaars was determined every month. Adequate care was taken to have control areas, the necessity of which was clearly shown during the operations; for example, the endemic index in the area of operations at one time fell rapidly, and it might have been concluded that it was the result of the antilarval measures, and, indeed, one could not have said it was not, had it not been for the control. But in the control bazaar, where no measures were being taken, a parallel fall in the endemic index took place. That certainly shows the necessity for control observations, which have been very rarely given in results hitherto published. You will have, I believe, two papers on Mian Mir, so that I need hardly discuss the matter. I must say, however, that I believe that Captain James and Lieutenant Christophers did everything possible to secure success, and that no expense was spared. The unfortunate factor that was not anticipated, namely, the flight of mosquitoes into the area from as far as two miles away, has been discussed by Captain James and Lieutenant Christophers in their report on Mian Mir. It was not that the antilarval measures were a complete failure, but that the conditions of the place made it impossible to prevent this influx. I am sure that nobody regrets the unfortunate result of this experiment more than Captain James and Lieutenant Christophers. There are many lessons to be learned from this experiment, hardly realized before, I think. I will mention two only (1) the seriousness and magnitude of the work, for the operations were conducted with the greatest perseverance for two years; (2) the fact that general amelioration of the sanitary conditions of the cantonments must play an important part in malaria prophylaxis, and that the removal of native bazaars with their infected population from the vicinity of European barracks is the main one to keep in view. How far antilarval measures are practicable where a network of irrigating channels exist remains to be seen, for apparently it was not this that was the cause of the Mian Mir failure, but an immigration from without that could not have been then predicted. It seems to me, then, very necessary that in the carrying out of measures of this kind the most careful consideration will have to be given to all existing conditions. That the great cause to which we all desire success has really suffered from the Mian Mir experiment, I do not believe. It has taught us useful lessons. The difficulties are greater than we at first perhaps suspected; that they will be overcome I have no doubt, but prophylaxis, be it of enteric, tuberculosis, or malaria, is necessarily slow in its progress. We should endeavour, however, to do our utmost to hasten it.

II.—Captain S. P. JAMES, I.M.S.; and Lieutenant S. R. CHRISTOPHERS, I.M.S.

THE SUCCESS OF MOSQUITO DESTRUCTION OPERATIONS.

So many operations where mosquito destruction has been successfully carried out are reported that it appears almost unnecessary to ask how far such measures may be considered

of practical utility in diminishing the incidence of malaria. It is, indeed, only when we make a closer study of the reports of operations that we realize in how many cases little or no proof of success is given, and in how many the conclusion of success is based on such inconclusive evidence as the number of admissions to hospital and popular report. As regards the number of breeding places it is necessary to destroy in order to bring about a successful result, we find curiously optimistic ideas very prevalent, and it is far from unusual to see a reduction in admission rates put down to the most trivial and inadequate operations. In perusing such accounts we find it very difficult to reconcile these easy and triumphant successes with the extreme difficulty encountered in our own efforts. The operations undertaken by the Government of India at Mian Mir¹ were throughout very thorough. Every effort was made to make the destruction of breeding places complete, and the experiments were continued for over two years. Many hundreds of pools were drained or filled, and throughout the whole area systematic search for and destruction of breeding places was consistently carried out. Here, then, if mosquito destruction is so readily brought about as would seem from so many accounts, we should have been able to show a very marked and decided reduction in the number of adult *Anopheles* and malaria. As a matter of fact the result in both cases was poor and totally out of proportion to the means employed. If adults were reduced in number it was demonstrable only in the first four or five weeks of the rains with the onset of which the fever season begins. Later on in the season it only required the care always necessary for the detection of *Anopheles* to make it very obvious that marvellously little reduction in their numbers had taken place. The effect upon malaria consisted in the delay of the onset of the fever season from the end of August to the end of September. In October the most strenuous and apparently complete measures against breeding places seemed not to influence malaria either in the bazaars or among the troops.

To what are we to ascribe the absence of the usual successful issue? We do not feel justified in considering it due to want of thoroughness in the conduct of the operations nor to peculiar conditions in Mian Mir. There is, indeed, in our minds little doubt that the difference is entirely due to the great disparity in the value of the tests used in estimating the reality of results achieved.

In very few instances that we know of has an attempt been made to verify a reduction in the number of adult *Anopheles*; nor has a reduction in endemic malaria, as shown by the number of infected natives, ever been demonstrated. The endemic index of Freetown approaches 100 per cent.² If measures really possess the success claimed for them it should not be difficult to reduce this to 50 per cent. or less. Short of a reduction in this percentage infection we cannot claim a success in the reduction of malaria.

In our reports on the experiment at Mian Mir we have attempted to give some idea of the determinable effect of the operations, and we succeeded in actually demonstrating such effect, though it was but partial.

Our attempts to really show what has been the effect of operations have a distinct value. They show that destruction of *Anopheles* sufficiently complete to be of practical importance as a prophylactic measure is an undertaking much in excess of the usual estimate of the character of operations necessary.

Perhaps the most valuable result of the Mian Mir operations was that they gave an insight into the difficulties which are almost certainly to be encountered in most really malarious places. We found *Anopheles* breeding places much more numerous than would be gathered from most accounts of operations, and the difficulty of keeping up with the ever-changing situation of breeding places under different degrees of rainfall was very great. We found that we had to deal with breeding places at considerable distances, and that quite local destruction was utterly futile. We encountered enormous changes in the facilities for breeding places in the rains, and had to count upon, what appears to have been the main factor causing our want of success, an infiltration of adults from outside the area which could not be prevented by any means in our power.

In chronicling our want of ready success we fully realize the inadvisability of damping enterprise by the suggestion of possible failure. But if in the near future we are really to reduce malaria in our tropical possessions, it seems to us

that something more must be done than merely to popularize the idea of anti-malarial measures. We see no reason to doubt that malaria will be reduced and in a manner much more direct than by blind schemes of drainage. Little good, however, seems likely to result from proclaiming a successful issue, on evidence which will not bear criticism.

Before concluding this brief note we may call attention to certain points which seem to us of importance.

1. It is easy enough to destroy larvae in millions and to do away with hundreds of breeding places. It by no means follows that malaria is diminished or adult *Anopheles* banished, and the success of operations on a large scale is still very doubtful.

2. The idea that by destroying breeding places around a barracks, prison, or other building, a reduction in malaria is likely to result requires more critical study before it can be accepted.

3. The occasionally-reported cases where a brigade has rid a town of malaria we have no hesitation in challenging as absurd. We are by no means so certain that the number of *Culex* and *Stegomyia* may not be very considerably affected by such means.

4. The use of "admission returns" of all kinds as evidence is a miserable method of proof, and the insignificant factors which greatly influence all such figures make them quite valueless for the purpose of estimating the effect of operations. The only really satisfactory test of success is a reduction of the endemic percentage of infection. In this connexion we recommend isolated bazaars and villages as more suitable in the present state of our knowledge for trial experiments than large tropical towns.

5. In any attempt to show a reduction in the number of *Anopheles* it is necessary to recognize that it is easy to delude oneself as to their absence, since they are always difficult of detection, and, except in certain places and in certain times of the year, would, as a rule, be considered absent by an observer imperfectly acquainted with their habits.

6. Mosquito operations reports are very often extremely uncritical and often ridiculous, since they claim an amount of success for which they have no evidence.

In conclusion, we hope that our remarks may not be without some result in directing attention to the need of more rigid experiments and the desirability of further knowledge being amassed regarding details, which should enable us eventually to form a special branch of sanitary science dealing with this important subject.

III.—RONALD ROSS, F.R.S., F.R.C.S., C.B.,

Professor of Tropical Medicine, University of Liverpool.

THE ANTI-MALARIA EXPERIMENT AT MIAN MIR.

THE President of the Section has asked me for some remarks on this subject: I must apologize for not presenting them in person.

The anti-malaria experiment at Mian Mir has been described in three reports, namely, one by Stephens and Christophers,¹ one by James,² and one by Christophers³ alone. The first, apparently written early in 1902, deals merely with some preliminary thoughts and observations, and I therefore do not propose to examine it here, beyond remarking that it clearly defines the object of the experiment which was then about to commence. That object was "to demonstrate by test operations" the "utility of all anti-malarial measures," particularly the destruction of *Anopheles* larvae (pp. 13 and 14). The actual operations and their results are detailed in the subsequent reports of James and Christophers; and with regard to these, I have the following remarks to make:

1. *Omissions*.—I can find no information in either report regarding several points of importance. Neither report is dated, nor contains a table of contents nor an index, nor gives a map of the country surrounding the small area of operations, a necessity for the reader. I can find no mention of the total population of Mian Mir, apart from the garrison—not even of that of the area of operations. Christophers gives the whole area of Mian Mir as being 12 square miles, and that of the selected area of operations as 4 square miles (pp. 2 and 8); but though the latter area is said to be "isolated," no indication is given of the degree of

¹ Reports to the Malaria Committee of the Royal Society, eighth series

² *Ibid.*, and also Scientific Memoirs by Officers of the Government of India, 1903.

³ Scientific Memoirs of Officers of Government of India, 1904.

¹ Scientific Memoirs of the Officers of the Army of India. New series, Nos. vi and ix.

isolation—that is, of the distance of the nearest villages or barracks.

2. *Organization.*—The organization of the work is not specifically described.

Was it directed by the Royal Society or by the Government of India, or was it only a private enterprise of the writers of the reports?

I gather that Captain James was in charge of the operations during 1902, and Lieutenant Christophers during 1903; but I can find only incidental remarks regarding the working staff at their disposal. James appears to have had the aid of a single hospital assistant (p. 34), and to have employed "between forty and fifty coolies" at one time (p. 24); but Christophers is quite indefinite on the subject. It is not stated whether the medical staff of the station assisted in examining blood films, checking admissions for malaria, searching for breeding places of *Anopheles* or superintending the operations. Yet such details are necessary to enable the reader to judge regarding the thoroughness of the work, because it may be questioned whether a single medical officer helped by a single hospital assistant (a native, I presume) could effectively attend to so many duties at once.

According to James (p. 34), "the greater part of the work of filling in pools and emptying out water from surface drains was done by contract labour, so as to avoid the necessity of constantly watching the coolies to see that they were working properly."

3. *Cost.*—In the matter of cost, I can find no statements as to who provided the funds, and as to whether any limit was put upon them or upon the duration of the experiment. The work was commenced on April 2nd, 1902, and James gives a "rough statement of expenses" up to the end of October (the seven months including the rainy season). The total cost was 7,217 rupees (about £521), of which 5,365 rupees (£397) were spent upon bricking and plastering a single watercourse, leaving only 1,852 rupees (about £124) for the remaining expenditure during seven months—an average of only about £18 a month. This included contract work, about £53 altogether; the wages of coolies, about £48, and contingent expenses for oil, material, etc., about £23. These sums, though exclusive of the salaries of the medical officer and his hospital assistant, were certainly not too large for dealing with a difficult irrigated piece of country during the rainy season. Logan Taylor¹ spent over £1,000 for a preliminary draining of the waste puddles of Freetown. Ismailia, though it was I think both a smaller and an easier area to deal with than that at Mian Mir, cost £5,000 for the first year.² According to Watson,³ Klang and Port Swettenham cost about £3,225 and £8,600 respectively for the first year's work. Sir William MacGregor tells me that Lagos has been costing about £10,000 per annum for anti-malaria work, including reclamation of swamps, tanks, building sea-wall, and mosquito-proofing rooms in Government houses. For Havana I have not been able to obtain the figures, but they were certainly considerable.

In Christophers' report of the work at Mian Mir during 1903 I can find no financial statement at all—a most unaccountable omission; but he is evidently sparing of oil for petrolage, because he says (p. 14): "Had all the pools in the area been oiled weekly, about 200 gallons, costing 50 rupees, would be required per month"—a little over £3. On the whole I incline to the view that he could have spent little more than £100 during 1903. At all events the expenditure at Mian Mir seems to have been extremely modest for so important an experiment, and one which was being conducted by the combined forces of such a wealthy body as the Government of India and such a learned body as the Royal Society, especially when we remember that it was an experiment the failure of which would tend to bring universal discredit upon a means of saving human life on the large scale.

4. *The Operations.*—The anti-mosquito operations are not easy to follow from the description. Commenced before the rainy season of 1902, they appear not to have been carried beyond a half-mile radius by the end of August, 1903. Thus Christophers says (p. 14): "Unfortunately, the great importance of some of these outlying pits, most of them at a distance of half a mile, was not at first recognized, so that it was only from the end of August, when larvae were found swarming in them, that actual measures were adopted." It is, therefore, scarcely surprising that mosquitos still occurred in the area

of operations up to and after that date. James (p. 3) mentions the existence of numerous wells, but we cannot ascertain how these were dealt with. I see also that instead of using oil freely (as has been done everywhere for years) he adopted baling as an alternative, because it might be said that oil damaged the crops (p. 21). Such caution was unnecessary, oil being used for spraying plants in green-houses; and baling would probably be an ineffective and costly substitute. During 1903, general oiling seems not to have been commenced until the end of August (Christophers, pp. 9 and 16), and then not beyond a half-mile radius. Moreover, it was repeated only every twelve days (p. 9), the reason for this being that Christophers thought that the aquatic stage of *M. rossii* lasted fourteen days. He says (p. 29): "Fourteen days appeared, therefore, to be the minimal time in which adult *M. rossii* hatched in September. As this month appeared especially favourable for the development of this species, it is probable that the above represents the shortest time in which *M. rossii* can develop."

On the other hand, James says (p. 30): "In this month (August) it was found necessary to repeat the oiling of pools at least every three days, as the larvae developed so rapidly and under the great heat the oil quickly evaporated."

Both cannot be right—if three-days intervals were required in August, 1902, twelve-days intervals would probably be insufficient in September, 1903. At all events, one may doubt the wisdom of risking in this manner the success of the whole undertaking merely in order to save a few pounds' worth of oil. Another question which arises is whether large numbers of larvae were not swept into the area by the influx of the irrigation waters, and the experiment of the closed *cul de sac* cited by Christophers (bottom of p. 16) obviously does not negative this supposition (see also James, p. 37). Judging from such details and from the small cost of the operations, I should say that the latter were not nearly so complete as the authors maintain.

5. *The Test for Mosquito Reduction.*—In an important formal experiment of this nature it was absolutely necessary to employ accurate and sensitive tests for gauging the number of mosquitos or the amount of malaria present. Without such tests small variations due to the operations would not be appreciable, and, indeed, the whole results might be called in question. Moreover, it was incumbent on the directors of the experiment not merely to state their personal impressions regarding the issue of the tests, but to give the actual figures arrived at—one of the first principles of reliable scientific work.

Now what was the test employed at Mian Mir for gauging the number of mosquitos? According to James (p. 18) it consisted merely in the formation of personal opinion by himself. He says: "It is impossible for an observer who has regularly collected *Anopheles* mosquitos in any given bazaar or barrack to ascertain with certainty whether they are more or less abundant in the year of operations than in previous years."

Christophers evidently relies upon the same test. But it is obviously open to the following objections: (a) the observer may be unconsciously biased; (b) only large variations in the number of mosquitos will be appreciable by this means; and (c) the actual ratio of reduction cannot be stated in figures. Moreover, even for the purposes of the roughest valuation, it would be necessary to state the number of times the test was applied; to give the number of houses searched, the number of control houses examined, and the number of occasions on which this was done. But neither James nor Christophers indulges us with this necessary information.

I see that this test is being hailed everywhere as being much more reliable than the one used by me at Sierra Leone, namely, "the general consensus of opinion." But, as will be seen by my report,⁴ I added to this consensus the special opinions of three competent observers, Drs. Daniels, Logan Taylor, and McKendrick. I was quite aware that, even with their support, our conclusions regarding the reduction of mosquitos at Freetown could not be considered definitely convincing; but then in my hurried visits to Africa I had no time to apply more stringent tests. On the other hand, Captain James and Lieutenant Christophers have been able to give years of study to the subject; and it is astonishing that they did not succeed in devising something better before undertaking so important an inquiry. As a matter of fact their test is exactly the same as mine except that being based upon the opinion of fewer observers it is, if anything, less exact.

6. *The Test for Malaria Reduction.*—Their principal test for

¹ *Memoirs of the Liverpool School of Tropical Medicine*, No. 5, Parts I and II, 1901 and 1902.

² *Ibid.*, No. 12, 1904.

³ *Journal of Tropical Medicine*, November and December, 1902.

determining the amount of malaria depended upon the microscopic examination of the blood of native children for the parasites. Theoretically it is a good test; but its practical application must present many difficulties. First, it is a general law that the percentage of infected children varies largely according to their age. This implies a careful classification of the children by age before the test is applied. Secondly, long search for the parasites is required in many of the cases; and this implies for accurate work a preliminary standardization regarding the amount of blood to be examined—obviously the percentage of children found to be infected will increase with the thoroughness of the examination. Moreover, the personal qualities or condition of the observer will come into play; he may detect the parasite less often if he is careless, unskilled, fatigued, unwell or biased. Thirdly, the well-known statistical errors have to be guarded against. If we examine 100 children under exactly the same conditions on three consecutive days we should almost certainly find a different percentage of infection on each day, and the difference might be large. Moreover, as in differential counts of leucocytes, a very large number of observations must be made before reliable percentages can be obtained. Fourthly, the apparent percentage of infection probably varies largely from season to season, apart from the influence of mosquitos. Thus I see from James's table (p. 6) that the admission-rate for malaria at Mian Mir begins to rise in April (especially among the native troops), and is already high in May; but according to him (p. 9): "It was not until the middle of May that adult mosquitos of this genus (*Anopheles*) became sufficiently numerous in the houses to be detected by searching." I have always inclined to the view that the parasites, both of man and birds, tend to be influenced by the external temperature. Lastly, even supposing that a locality were entirely freed from mosquitos, it is doubtful whether a marked reduction in the percentage of infection would occur for months, or perhaps years, at least without quinine medication: so that any expectation of finding an immediate marked reduction after a few months' partial clearance of mosquitos appears to me, as I have often stated, quite unjustifiable.

This test must therefore be a most difficult and laborious one, at least for the detection of small variations in the malarial endemicity. Unfortunately neither of our authors seem to be even conscious of the difficulties just enumerated. They seemed to expect a marked reduction *pari passu* with the operations; they give no adequate details of the examinations, and worst of all often content themselves with publishing mere ratios (James, pp. 7, 43, 44). Christophers (pp. 20 and 21) scarcely gives any figures at all regarding the endemic index, and merely states that it was not diminished. The military statistics which he quotes are almost worthless, because no effort is made to distinguish new infections, and because the previous health of the troops is not exactly discussed.

7. Some Startling Results.—Let us now examine our authors' statements regarding the effect of the operations on the number of mosquitos within the area. Christophers states (pp. 5, 17, 36) that "in spite of most energetic measures," the destruction of "millions of larvae," and "the almost complete absence of breeding," the "reduction in adult *Anopheles* was at the most very slight." He described this as a "somewhat startling fact." Indeed, the insects seemed if anything to have increased; for he says (p. 36) that in spite of the operations "the adults still appeared in large and in increasing numbers in the area"; and James says (p. 42) that "the number of adult insects present in the houses increased considerably instead of diminishing." Yet Christophers says (p. 16): "In the operations the destruction of larvae was enormous. A single oiling of a canal in the later months must have destroyed millions, since it was not unusual to remove fifty or more specimens in a single dip of the dipping can."

Let us picture to ourselves the real meaning of these statements. Here was an area of four square miles over which propagation was almost completely arrested for a year and a half. The destruction of larvae was so immense that millions were killed in a single oiling of a single canal, and we are assured that all the waters were oiled at least once in twelve days. How many insects must therefore have been destroyed during the whole period! Yet the effect of this colossal slaughter was inappreciable—was, if anything negative. In other words, if all these millions had been allowed to come to maturity it would have made no practical difference. How immense, then, must be the total number of *Anopheles* in unfortunate Mian Mir!—there must be many

millions of them to every man in the place. I confess that I have never seen or heard of such a thing before. One sometimes finds a few score mosquitos in a house; but at Mian Mir each house must contain myriads.

But there is one—and only one—other possible explanation of these statements, namely, that as soon as the slaughter commenced an enormous compensating influx of *Anopheles* must have set in from without. And this influx must have begun with the operations in order to maintain the balance of *Anopheles* within the area.

What caused such a curious phenomenon? Were the insects attracted by the smell of oil, or perhaps by the perspiring coolies baling out the puddles? Perhaps they flocked in to watch the operations. Indeed James (p. 38) describes them riding in "in carriages, carts and other vehicles from neighbouring towns and villages."

Seriously, Drs. Christophers and James must face these alternatives, both unlikely if not impossible—either there are such enormous numbers of *Anopheles* at Mian Mir that the destruction of many millions of them makes no difference, or else a great compensatory immigration must have commenced when the operations commenced.

But many others of their statements may be criticized. For example, Stephens and Christophers say (p. 17): "The results of the examination of the children of different bazaars show a correspondence between the proximity of ditches and the malarial index." James (p. 12) confirms this, and adds that the number of adult *Anopheles* "that can be caught in the different bazaars and lines varies with the distance from a breeding place"—he means "varies inversely as the distance." On p. 38 he records an interesting experiment. "As we had already shown," he says, "that the further a group of houses is removed from a breeding place the fewer are the adult insects present, my plan was to remove all the inhabitants of one series of syce lines to such a distance from all breeding places that no *Anopheles* mosquitos at all should be present in the houses on the new site." Accordingly a site was chosen "over 600 yards from the nearest pool," and the inhabitants of the syce lines were moved into it in August, 1902. "As a result of this no adult *Anopheles* mosquitos were found in any of the tents during September and October;" and (p. 45) the endemic index fell from 56 in the October of the previous year to 1 child out of 25 in the October after the removal (a somewhat sudden fall). Of course this accords perfectly with the experience of everyone; and indeed it would be mathematically impossible for mosquitos to be as numerous far from their breeding place as they are near to it; the mosquito-density must diminish with distance as radiated light and heat diminish. Hence the nearest breeding places must, *ceteris paribus*, be the most dangerous. The fact that local malaria is connected with local marshes and may be removed by local drainage has been known for ages—evidently owing to popular observations of instances similar to those recorded by James. Now, however, we find Christophers remarking (p. 11): "Our experience in Mian Mir has led us to look upon measures confined to the pits and puddles in the immediate neighbourhood of buildings as quite futile;" while on p. 36 he makes the sweeping and astonishing declaration that "the mere obliteration of local breeding places is useless." Surely there must be something wrong here. If the obliteration of proximate pools is quite futile, as he says, that of distant pools must be less than quite futile, so that the drainage of all the pools in the world would have no effect. As a corollary, these wonderful mosquitos must either breed without breeding places or be immortal.

I am sorry to see that Dr. Christophers disposes of another prophylactic measure against malaria, and one which he has already advocated hitherto, namely, segregation. Referring to the sporozoite rate in *M. culicifacies*, the principal malaria-bearing mosquito of Mian Mir, he remarks (p. 28): "It matters little in this respect from which house, shed, or stable this species is taken, since the percentage of infected individuals is much the same in all places." Segregation would therefore be obviously useless, at least against *M. culicifacies*.

Christophers concludes by remarking that "the destruction of *Anopheles* within an area by attacking their breeding places is extremely difficult." One asks at once, what does he really mean? He apparently expected to banish all the mosquitos within his area of operations for a few hundred pounds. But what may be difficult to effect for so small a sum may not be difficult to effect by means of a proper expenditure. Malaria at Mian Mir must cost the Govern-

ment alone some thousands of pounds annually, representing the interest on perhaps fifty to a hundred thousand pounds capital; and it is therefore a sum of this magnitude, and not a poor hundred or two, which for economical reasons alone may be spent on banishing the disease there. A task is easy or difficult according to the power available for performing it. Again, what does he mean by the "destruction" of *Anopheles*? Does he mean complete extinction or partial reduction? The former must of course always be more difficult and costly than the latter. It is a question of degree. To state that the task of destroying mosquitos is extremely difficult, without specifying either the degree of destruction or the means available for effecting it, tends merely to discourage future work, without really conveying any exact meaning whatever.

After discrediting most of the antimalaria measures suggested by others, Dr. Christophers hints that we may yet find salvation in some "gradually carried out sanitary reforms," but with commendable caution he fails to satisfy our curiosity by explaining what these reforms are. We can imagine the delight with which this judgement will be hailed by all local authorities. "No more trouble, no more expense," they will cry; "we know quite well what the words sanitary reforms mean, even without looking in the dictionary—they mean night-soil buckets—let us buy some more at once, they are quite cheap."

On p. 17, Christophers says: "The outcome of our experiment with regard to the effect of antilicidal measures against *Anopheles* is therefore at direct variance with the results of such operations as have hitherto been recorded. It is but fair, however, to add that in many of such recorded operations no attempt whatever was made to show by search in the houses that the number of *Anopheles* was really diminished."

It would have been still more fair if he had shown justification for this statement. Such search was of course made at Sierra Leone, and, I suppose, everywhere else. The "direct variance" is more probably due to a much more evident cause.

8. *Summary*.—My remarks may therefore be summed up as follows: (1) We are not convinced that mosquito propagation at Mian Mir was really suppressed to the extent claimed, even over the small radius of half a mile; (2) the test employed for detecting reduction in the number of mosquitos consisted of nothing but the formation of a personal impression unsupported by definite figures, and was therefore neither delicate enough nor convincing enough for the purposes of a formal and important experiment like this one; (3) the figures given regarding variations in the amount of malaria are not sufficient to enable us to form any opinion one way or another; (4) several of the statements contained in the reports appear either to be contradictory or unintelligible, or to involve mathematical impossibilities; (5) even if we admit the thoroughness of the work done and the validity of the tests used, the whole experiment is still open to the final criticism (a) that it might not have been continued long enough, and (b) that the radius of operations might not have been large enough.

I therefore think that the sweeping conclusions placed at the end of Lieutenant Christophers' report are not supported by the evidence which he and Captain James have published. They are not entitled to claim any more than is contained in the following sentence: That after operations extending to a half-mile radius, lasting a year and a half, and (apparently) costing between two and three hundred pounds, no very large reduction in the number of mosquitos or in the amount of malaria was, judging wholly by their own personal impressions, effected. I can quite believe it.

Owing to the omission of a financial statement for 1903, we can only estimate the cost given above. The considerable sum spent on bricking the watercourse is not included, as it seems to have been more or less useless (Christophers, p. 4, line 28); probably a few pounds' worth of oil would have done as well.

In these remarks I have confined myself to a textual commentary and have not dealt with the broad principles which govern the prophylaxis of malaria, and which, though self-evident enough, require a more or less mathematical treatment for their formal demonstration. The logical basis of the great measure of mosquito reduction is absolute. There is no doubt whatever that in any locality we can reduce mosquitos to any percentage we please, provided that we arrest their propagation to a sufficient degree and within a sufficient radius. This proposition, like the multiplication table, does not require experimental proof, and is incapable of disproof.

If experiments would appear to disprove it, the experiments and not the proposition must be wrong.

Experiment is required, not in support of the general principle, but only in order to obtain certain unknown constants. We have still to determine (a) the radius of operations required to reduce the density of a given species of mosquito to a given percentage: and (b) the percentage of mosquito reduction required in order to obtain ultimately a given percentage of malaria reduction. But experiments directed to this end must be of a true scientific quality; they must be prefaced by a mathematical inquiry and be executed by means of rigid tests applied by the brain as well as by the hand. In the meantime, we may rest assured that the smallest work against malaria will not be without its reward. The oiling or draining of a single pool, the cinchonization of a single person, and the "proofing" of a single room must and will perform their small quota of good. If we can do more than this, let us do it; we shall undoubtedly get better results. But if local authorities wish to clear the malaria in a large town or cantonment they must be prepared to spend money on the work, just as they would for a new water supply or sewerage system. The example of Ismailia is always before them. The road leads straight forward. Work like this at Mian Mir only tends to arrest enthusiasm in the cause without really adding anything definite to our knowledge.

IV.—CAPTAIN E. P. SEWELL, B.A., M.B., B.C. Cantab.,
R.A.M.C.

THE RESULTS OF THE CAMPAIGN AGAINST MALARIA IN MIAN MIR.

Topography.

THE cantonment of Mian Mir is a little over four miles from Lahore, the capital of the Punjab. It is built in the midst of an arid, almost treeless plain. The soil is alluvial sand, with an admixture of clay. At a depth of about 6 ft. is a layer of "kunkur," a hard, stony material which is used for road-making, and is almost impervious to water. The fall of the ground is very slight, and the kunkur layer hinders the percolation of water through the soil, so that, after a heavy shower of rain, a large part of the station lies under water. The resulting pools often take several days to dry up, and during the monsoon, when they become replenished by frequent showers, the larger pools remain for months at a time, and afford suitable breeding places for the *Anopheles*.

The nearest river is the Ravi, distant about seven miles to the north. From the head waters of this river water is brought by the Bari Doab Canal for irrigation purposes. This canal passes within a mile of the cantonment, and from it several smaller irrigation channels take their origin and pass through the station, supplying each compound with water by means of lateral branches. These latter are mere ditches, in many cases overgrown with weeds and grasses, and in them the larvae of *Anopheles culicifacies* can be found in plenty throughout the summer and autumn months.

Irrigated crops—cereals and cotton—are grown not only in close proximity to the cantonment, but around and between the individual bungalows. Even rice crops are grown within a mile of the barracks.

Climate.

The climate is for the greater part of the year very dry. Nearly the whole of the annual rainfall occurs in July, August, and September, the monsoon usually breaking in the middle of July, and continuing with longer or shorter "breaks" until the middle of September. The average annual rainfall of the last fourteen years has been 18.2 in. The temperature varies considerably; while in the summer a maximum shade temperature of 120° F. is often registered, it is not uncommon to get a minimum of 25° F., or even less, in the winter.

Mosquitos.

Throughout the warmer months mosquitos, chiefly *Culex*, are very numerous. They begin to appear at the beginning of April, and their numbers rapidly increase until the weather gets hot, when they fall off a little, only to become more numerous than ever during the rains. Very few are seen after the beginning of November.

Anopheles are not found in any numbers in the bungalows until the rains, although they can be found in small numbers in the bazaars. But during the rains they are present in large numbers in most of the bungalows, barracks and hospitals. The commonest forms are the *A. rossii* and the *A. culicifacies*. The larvae of the latter are found from April to October in running water along the weedy margins of the

smaller irrigation channels, those of *A. rossii*, on the other hand, in pools of water formed by the rains, and so are not present in any number until the monsoon bursts in July.

The Carrier of Malaria.

Of these two species, the *Anopheles culicifacies* has been found by Captain James, I.M.S., to be the carrier of the malarial parasite. The habitat of this mosquito being the irrigation canals, it is natural to ascribe to the irrigation system the malarial unhealthiness of the station.

Malaria in Mian Mir.

The following facts strongly support this inference. The Bari Doab Canal was excavated and the irrigation system introduced into the cantonment in 1867, in which year the fever rate jumped to 1288.3 per 1,000, from 654.3—the average of the preceding seven years. In the thirty-five succeeding years the high rate of 1386 per 1,000 was maintained.

These figures represent the number of admissions to hospital per 1,000 of British troops for malarial fevers. They by no means accurately represent the actual amount of malaria, since on the one hand numbers of mild cases are only detained and not admitted to hospital, and are therefore not shown in the returns, while, on the other hand, they include many cases of readmissions for recurrences. However, they serve for the purpose of comparing one year with another.

It is clear, then, that the incidence of malaria among British troops has more than doubled since the introduction of irrigation, and there can be no doubt that irrigation is the chief cause of the unhealthiness of Mian Mir.

Effect of Rainfall.

There is another factor, however, which largely influences the amount of malaria in any particular year, namely, the amount of rainfall. For instance, in 1899 there were only 6.21 in. of rain, and a malarial rate of 46.9 per cent., while in 1900 22.07 in. of rain were registered and a malarial rate of 107.9 per cent.

Antimalarial Measures.

Any measures, then, which have for their object the improvement of Mian Mir, should be directed mainly against the irrigation systems, and should also provide for the proper draining of the cantonment, and the filling up of all holes, pits, and low-lying ground upon which water remains after a shower of rain. It was on these lines that Captain James, I.M.S., in 1902, and Lieutenant Christophers, I.M.S., in 1903, prosecuted a campaign against malaria in Mian Mir. The whole station, however, offering too wide a field for their experiments, the south end, which includes the R.A. lines and the Military Prison, was chosen, as being of a suitable area and somewhat isolated. Their efforts consisted mainly in having the irrigation canals systematically cleaned, the banks made even, vegetation completely removed, and the sides plastered with mud, so as to present a smooth, unbroken surface to the water. Pools left when the water was shut off were baled out and allowed to dry. Oiling was practised whenever larvae were found to be present in any number. During the rains large numbers of shallow, rain-formed pools, which were found to contain larvae of *A. rossii*, were drained, oiled, or in many cases filled up. Bungalow compounds were kept under supervision, holes filled up, and empty tins removed.

Results of the Operations.

The way is now clear to examine the results of the operations, as shown by the malarial rate among British troops in the station.

The following table gives the malarial rate per 1,000 for the years 1902 and 1903, and compares them with the average of the preceding ten years:

1892-1901 Average.	1902.	1903.
932.1	269.4	1031.7

1902.

It will be seen at once that there was a large decrease of malaria in 1902, and the credit of this was somewhat prematurely ascribed to the antimalarial operations, but it must be borne in mind that the year was one of light rainfall, well distributed throughout the year, and that other stations in the Punjab were in the same healthy condition. Moreover, the reduction was just as well marked at the north end of the station, where no extraordinary efforts were made to fight the

disease, as at the south end. The most that can be said is that in 1902 the efficacy of the operations was not proved.

1903.

The year 1903, on the other hand, was a malarious one throughout the Punjab. The rate at Mian Mir rose to 1031.7, a figure which is in excess of the average for the ten years preceding the experiment. Not only were the number of admissions high, but the type of fever was very severe. Haemorrhagic cases were very common, and several cases of the comatose cerebral form occurred. Several cases of the algide type might almost have been mistaken for cholera. All these malignant types are fortunately uncommon in Mian Mir.

It is difficult to estimate if any benefit to the station as a whole was afforded by the operations, as it would be fair to argue that the state of affairs might have been still worse were it not for these operations. But if the operations were a success it should at least be incontestable that the health of the area included within the scope of the operations was better than that of the area outside. The opposite, however, was the case. The malarial rate among the troops occupying the south end was more than double that of those living at the north end.

The following table shows that the south end is not usually to any extent more unhealthy than the north end:

Malarial Rate per 1,000.

Year.	South End R.A. Lines and Military Prison.	North End. British Infantry.
1900	148	529
1901	90.6	702
1902	73	54
1903	169	71

The facts of the case, then, are, that in 1903, the second year of the antimalarial campaign, malarial fevers were in excess of the average both in the number of cases and the severity of the type, and that this excess was more pronounced within the area of the operations than in the remainder of the cantonment.

Causes of Failure.

With these facts in view we can only conclude that the campaign has up to the present proved a failure, and the cause of this failure is not far to seek. The operations were doubtless efforts in the right direction, but were conducted on a scale utterly inadequate to make any impression on an evil of such a magnitude as that against which they were directed.

At the outset, it was a mistake to attempt to treat the R.A. lines as an isolated area. They cannot be treated as such, and any factor which affects the rest of the station is bound to affect a part of it, which is not separated by any physical division but is in direct continuity and subject to the same influences.

Secondly, any plan which aims at the reduction of malaria in Mian Mir must have as its basis the entire abolition of the irrigation systems, and the clearing off of all watered crops in the cantonment, and around it for the distance of at least one mile.

Thirdly, although the operations included the filling up of certain pits and holes and the draining off of water from low-lying patches of ground, a few minutes' acquaintance with Mian Mir after a heavy shower of rain would convince any one of the utter inadequacy of such local measures in one corner of the cantonment. There was nothing to prevent the *Anopheles* breeding in any of the numerous pits and ditches outside the area, and flying a few hundred yards to the buildings within the area.

To meet with any success a campaign against malaria must be on a very much larger scale. Nothing short of the complete levelling and draining of the whole cantonment and its environments, and the total abolition of irrigation and irrigation channels in and around the station is at all likely to cause any appreciable difference in the amount of malaria in Mian Mir.

Unfortunately these drastic measures involve considerable expense. The stoppage of irrigation would entail great loss to the zemindars, and the compensation paid to them for the deterioration of their land and loss of crops would amount to a large sum of money, which local funds are quite unable to meet, and which the Government of India has hitherto shown no disposition to grant.

Appendix.

Year.	Rainfall.		Percentage of Malaria.
	Monsoon.	Total Annual.	
1892	20.32	23.51	107.1
1893	14.71	26.00	94.9
1894	9.24	23.19	136.5
1895	5.80	12.16	148.3
1896	6.53	9.91	30.8
1897	13.45	20.25	73.8
1898	11.42	18.44	80.8
1899	3.88	6.21	46.9
1900	18.94	22.07	107.9
1901	12.78	17.96	135.4
1902	8.90	11.83	26.9
1903	11.92	14.71	103.2

V.—HENRY STRACHAN, C.M.G.,

Principal Medical Officer of Lagos, West Africa.

I FEAR I can say nothing that will not be said, and said better, by others on this important subject. But I cannot refuse to give my views, such as they are, when requested to do so. Our present knowledge, founded partly on the soundest scientific investigation, and partly on the results of actual practice—which is based on the former—during the past few years, enables us to declare, and with emphasis, that the prophylaxis of malaria resolves itself ultimately, so far as the individual is concerned, into obedience to the rule, "Do not permit yourself to be bitten by mosquitos," simple to enunciate, but very difficult to obey in such places as the West African Colonies.

The measures necessary to carry this rule out thoroughly, and in the fullest sense, obviously include such as have for their object the destruction of existing adult mosquitos, and the prevention of their presence in a given locality by rendering the breeding of them there impossible, and by destruction of larvae.

Measures taken for the destruction of existing mosquitos involve an expenditure of money and time that place them almost entirely out of the range of practical work. In Lagos we have tried the experiment of employing a "mosquito catcher" in certain large institutions, but I am not able to say conscientiously that the results have been very valuable. The reduction of the number of mosquitos of various kinds cannot have been greatly aided by this means, as a constant influx of new individuals replaces the few destroyed daily, though possibly in a few places the comfort of residents may have been a trifle increased.

A. But the prevention of the presence of mosquitos by destruction of all possible breeding places and of the larvae, is a matter of vital importance, and on it our hopes of ultimately abolishing malaria in West Africa mainly depend. It includes:

(a) Filling up thoroughly of all areas of depression in the surface of the land that cannot be adequately drained, with the planting of water-absorbing trees.

(b) Filling up of swamp areas.

(c) Drainage of all areas where filling up is impracticable, but drainage is possible.

(d) Elimination of all the smaller breeding grounds (preferred by *Anopheles*), such as:

1. Domestic water receptacles, jars, calabashes, etc. (whose contents are left long enough to allow the short time necessary for breeding of the insects), water-butts and tanks having orifices open to admit the ovipositing mosquito. The former should be emptied daily, the latter two carefully closed at all joints, and the necessary orifices guarded with fine wire gauze.

2. Puddles in compounds and streets should be filled up, or if that be not possible during the rainy season, swept out daily, or treated with paraffin oil.

3. During the rains all plants whose leaves are so constructed as to collect and retain water where mosquitos breed (such as bananas, bromeliads, pineapple plants, yuccas, and the like), and hollows in tree trunks, or where branches have decayed away, should be carefully inspected, and, if necessary, removed when close to human habitations.

4. Empty tins, jars, broken bottles, potsherds, even pots in which plants are growing, and where heavy

rain allows water to remain for some days above the surface of the contained earth in them, must be removed.

5. Flower vases must be daily emptied to prevent *Anopheles* from breeding in them in the house. (I have seen more than one instance in which no breeding ground for *Anopheles* could be found after weeks of search in the immediate neighbourhood, where the source of supply of the insect was eventually traced to the flower vases decorating the dining table!)

(e) Attention to wells. These need:

1. Careful closing, with employment of a pump outside of an enclosing railing. (This also aids in preventing pollution of the well water.)

2. When the latter is impossible, covering with a well-fitting lid, and frequent inspection to ascertain presence of mosquito breeding.

3. When larvae are found. Use of "larvicide" (which though somewhat disagreeable, is not poisonous to human beings), or of kerosene oil, with subsequent cleaning out of the well.

(f) Attention to ornamental fountains and tanks. These can be emptied, or treated with kerosene oil, when larvae are found in them. Fish will sometimes prevent breeding of mosquitos in such places.

(g) Attention to gutters under the eaves of houses. This, too, is of importance, and is sometimes overlooked. Gutters, when at all irregular, contain small collections of water which last long enough to enable mosquitos to breed in them. They should be regularly inspected and swept out, and defects of structure and level corrected. (Iron in the water is not inimical to *Anopheles* larvae. I have seen small collections of water in iron vessels which were almost solid with iron rust and larvae!)

(h) Attention to cesspits. They must be treated with kerosene oil when larvae are found breeding in them.

B. Equal in importance to the prevention of mosquito breeding is the employment of means to prevent the adult insect from attacking persons. They are:

1. The careful use of the mosquito net.

2. Use of mosquito-proof rooms.

3. Use of punkahs (or electric fans), fly-whisks, and other mechanical devices for warding off the attacks of the insects.

4. Burning, and other use, of certain materials (chiefly containing pyrethrum, or essential oils) in rooms, on the person, etc.

The use of the first is most important, since it is simple, within the power of all, and very effectual during the night hours.

The use of the second (mosquito-proof rooms) is really of the greatest importance, since it is in the evening hours, before going to bed under the protecting net, that the risk of infection from mosquitos is, perhaps, at its height, but in practice is most difficult to carry out—in Lagos, at least. The greater degree of heat felt in a mosquito-proof room is apparently considered by many to be worse than an attack of malaria, and the use of the mosquito-proof room is evaded.

This leads to consideration of the use of punkahs and fans. They are not available to the mass of persons in such a place as Lagos. Only a few Europeans can afford to employ the former, as there is no class of native here to accept punkah-pulling as an occupation, as in Eastern countries, and the natives will not use such means themselves. Electric power is not supplied by Government to all officers' quarters, and, of course, by the vast mass of the population it is impossible of employment. The fans, too, are somewhat costly articles. So that the heat in a mosquito-proof room is not to be mitigated, except in a few rare cases by those mechanical aids.

In consequence of this, as I am informed, many persons will not use those mosquito-proof rooms which have in some cases been supplied; so that a most important means of prophylaxis does not at present promise to be of much avail in Lagos, except to the very few who now make use of it.

I have suggested that malaria should be treated legally, as other infectious diseases are, so that the person who does not care enough for his own health to avoid risk of infection shall, when suffering from malarial fever, be deemed to be a source of danger to others, and therefore be isolated until recovery. This practice would in time lead such persons to take extra precautions against being infected. And costly mosquito-proof rooms would not be left to decay, unused by

those to whom they may be supplied, as would seem to be their fate now.

At the same time I feel bound to say that such rooms should be constructed on a better plan than they seem to have been, taking them generally. Some allow for easy ventilation during the hours of the day; others are practically permanently sealed, and when not exposed to a very strong breeze the heat is really unendurable, and one cannot be surprised that men refuse to live in them. Others are so situated as to be flooded during the rainy season.

Also one does not forget that even those persons who are scrupulously careful in observing all the means of prophylaxis are at times exposed to infection from no fault or carelessness of their own.

The use of burning pyrethrum, of essential oils, and so forth, must be considered among the minor accessory means of preventing attacks. My experiments a year or so ago with *ocimum viride*, a plant which was said to be so greatly disliked by mosquitos that its presence rendered theirs impossible, proved to me that it was quite useless. I did not publish the observation, as Dr. Prout, of Sierra Leone, coming to the same conclusion, pointed out the fact in the press.

Among accessory measures must be considered:

(a) Avoiding the heat of the sun during the hours when its power is greatest (say from 10 a.m. to 4 p.m.). One cannot but regret that young officers—military—are exposed during these hours on the very hot rifle range or at parade. The result is frequent "slight touches of fever," as they term it—and an undermining of strength that lessens resisting power. These "touches of fever" being either sun-fever or malaria, which is not a fresh infection.

(b) Avoiding of chill, from cold or wind or wetting by rain. Many cases of fever (not re-infection) are brought about by these agencies. The refreshing temporary effect of a cool breeze causes the individual to "enjoy" it until he is chilled. This is very noticeable on ships returning to Europe.

(c) A mode of life in which "temperance in all things" is the guiding principle.

C. Lastly, there is to be considered prophylaxis from employment of drugs. The action of quinine is too well-known to need discussion. But some twenty-two years of observation of the use of this drug leads me to entertain the following opinions as to its value and limitations.

It is useful—in daily small dose (5 or 6 grains), or in a larger dose (6 to 10 grains), on one or two days in a week—in preserving some persons from malarial attacks, under conditions where such attacks might be expected, or if they are attacked, in causing the disease to run a brief and mild course, generally speaking.

Others, however, are attacked in spite of such regular taking of quinine.

There is a notable percentage of persons, who cannot take quinine without marked suffering in various ways, especially disturbances of digestion and of the nervous system, and perhaps, after prolonged use, changes in the blood and the nerves, such as those of sight and hearing.

The usual preparations of quinine are therefore useless to such persons as a prophylactic. Other preparations (as en-quinine) and other drugs (as methylene blue and arsenic) have been from time to time suggested for employment in such cases, but so far have proved of far less value than quinine.

Recently thiocol has been advocated, and I am inclined to think with some reason, but the number of cases in which I have used it, apparently with good effect, is at present too small for me to base on it any useful opinion; I, however, think it would be well if the drug were widely tested with the view of settling the question. It has the advantage of being easily taken, and of not disturbing the well-being of the taker.

In view of the cost of quinine (a minor disadvantage in one sense, but one to be considered where thousands of natives are concerned), and of the drawbacks mentioned above to the use of the drug, I am compelled to regard its use as a prophylactic, though so very valuable, as being secondary to its value as a means of destroying the malaria parasite. For it is in antimalaria work on a large scale that this, its greatest value, is evident, it being the one reliable agent known for destruction of the parasite in infected persons, and so, ultimately, for reducing the amount of the malaria poison which is in existence in any given region; and for this reason its use is to be urged.

While, therefore, I advocate its employment among the known means of prophylaxis, I feel that those which relate to protection from mosquito-bites are the ones from which we may expect the most marked and widespread results in Lagos in the immediate present, since reduction of the amount of infecting material (the parasite) must be a matter of years, considering the vast numbers of infected natives in the large areas inhabited by them.

It is necessary to consider the prophylactic measures that should be taken by persons travelling in the Bush, away from their residences.

I have found the little apparatus described below, made for me by Messrs. Benjamin Edgington, most useful in warding off mosquito attacks in the evening hours and at dawn—the periods of the day when one is most exposed to such attacks.

A large umbrella, of waterproof canvas, with cane ribs and a bamboo support. (This opens widely enough to allow room for small table and chair and one can take meals, and sit and read or write in comfort, protected from mosquitos and the other myriad insects which, attracted by light, make the taking of meals a very uncomfortable proceeding at certain times of the year.) Over the top of this umbrella fits a circular piece of calico, from the edge of which to the ground hangs a weighted and ample mosquito-net. The entrance is made by wide overlapping folds of the netting. This apparatus is light, easily carried about, and is useful in many ways. From this umbrella-net, one proceeds to bed, where the usual mosquito-net at once affords protection during the night.

In the camp-beds in vogue the rods to support the mosquito net give, after very short use, and allow the net to come in contact with the sleeper's body. I have obviated this difficulty by having the brass rods pass right through the edge of the cot into holes in the four corner legs, so that they are tilted outwards and so keep the net away from the sleeper. Both these little improvements have been made by Messrs. Edgington at very moderate cost, and will be found, I am sure from my experience, to be boons to the traveller. Quinine—3 gr. of hydrochlorate—should be taken daily when on the march. I need not dwell on the necessity for choosing, when possible, suitable sites for camping, nor on precautions to be taken as to water-supply.

From the foregoing it will be seen that in most places the antimosquito war (and in consequence the antimalaria war) becomes a mere question of money expenditure—money expended in preventing mosquito breeding. Given the necessary funds we may expect excellent, not to say marvellous, results—as in the case of Havana, to quote one striking instance.

In West Africa there are places where great improvement may be expected—Lagos Island is such a place, but where complete success cannot be hoped for, owing to physical conditions. Such places are better abandoned, in my opinion, and a new town built in a suitable site.

This is not the time or place for me to discuss the enormous advantages that would follow such a move in the case of Lagos Town. Apart from the malaria question, the obtaining of a fair water supply, of soil where vegetables and fruit could be grown and good cattle and sheep reared for food, and of a locality where new and good sanitary arrangements could be at once established—all so sorely needed in the present town—would revolutionize life for Europeans. But the all-powerful god, Mammon, places his veto on such a life-saving migration, and all that is actually in our power is to do the best possible to improve existing conditions in the existing town.

It will, perhaps, be noticed that I have not considered the question of prophylaxis as affecting Europeans apart from natives.

Apart from the humanitarian and political reasons, and apart from that duty towards the native which is very properly a portion of the "white man's burden," all of which are opposed to segregation, I consider that even from a lower and a selfish point of view, segregation is to be avoided. The amount of existing malaria poison must be reduced, and that could not be, if only segregation were practised, without constant efforts to improve the condition of the native in regard to this great cause of suffering and death among them.

There is, to some extent, a natural segregation—"like will to like"—and so the European quarter is in most places somewhat separated from the native, to the satisfaction of some of both sections of the inhabitants no doubt; but even so intermingling must occur, and I feel it is as impossible as it is inexpedient for high or low motives, to endeavour to create a wider breach between native and European.

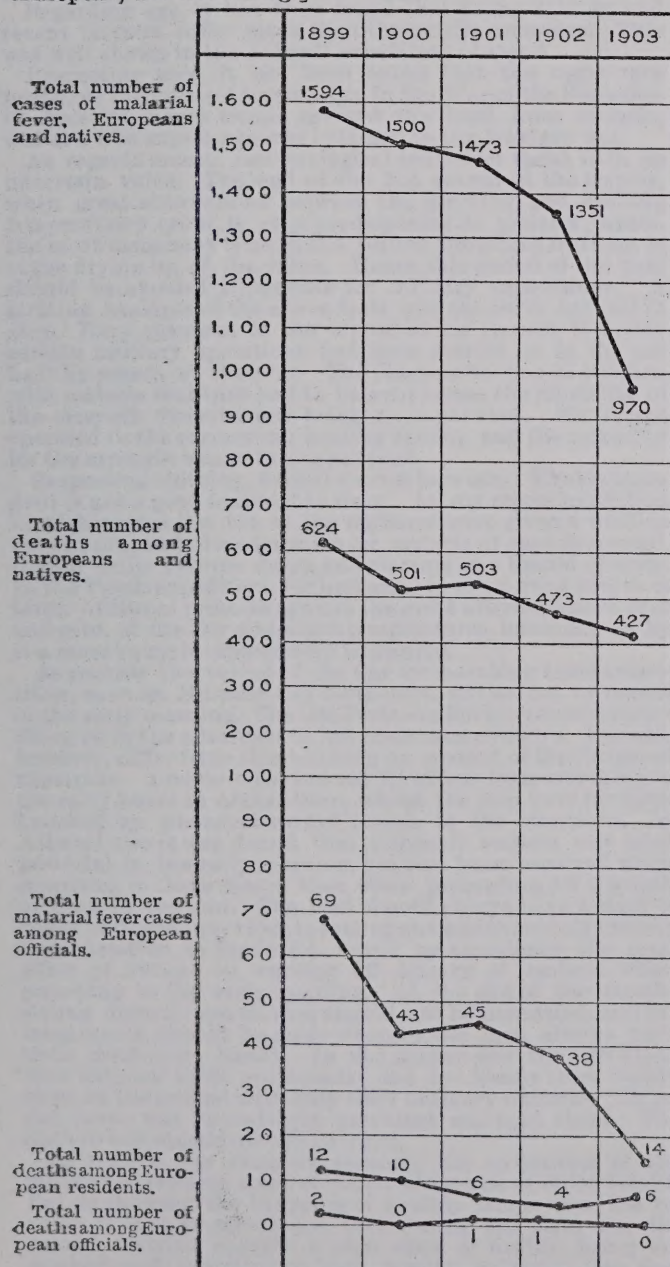
In Lagos during the past few years we have endeavoured by illustrated lectures and pamphlets, by the teaching of elementary sanitation in schools, and by the constant intervention of trained sanitary inspectors, to educate the natives in regard to malaria, its cause and prevention, and I venture to think the records of the past six years show that such education is having the desired effect, though it must be many a year before very marked beneficial result will be clearly in evidence so far as the native is concerned. Quinine has been issued regularly to all officials, European and native, and to such other natives as would take it, for the past three years.

The attached chart gives the number, so far as reliable records show them, of cases of malaria among Europeans, European and native officials, and the death-rate among natives, during the past five years in Lagos.

LAGOS, WEST AFRICA.

Total Number of Malarial Fever Cases, including Deaths, recorded among the Europeans and Natives (Officials and Non-officials) from 1899-1903 inclusive.

[During these years the population, both native and European, was increasing.]



amount of obedience to advice and greater precaution against infection than is the case with other European residents, who were more prone to follow their own ideas as to prophylaxis and treatment. I trust that in the future we shall see as great a degree of improvement among them as among officials.

Blackwater Fever (Europeans).

	1899.	1900.	1901.	1902.	1903.
Total cases	5	2	9	11	4
Total deaths	3	6	4	5	2
Of these cases there were among officials...	1	1	4	1	3
Deaths	0	0	0	1	1

This chart is, I think, of great value, as indicating the changed condition as regards number of cases and deaths (malaria) in Lagos town since the end of the year 1898, when I arrived in the colony.

The number of cases, European and native, has fallen steadily each year, from 1,894 (in 1899) to 970 (in 1903), while the number of deaths has fallen from 624 to 427 in the same time. Of these deaths, the majority, of course, are among natives, for the deaths among all European residents have fallen from 12 to 6, and among European officials from 2 to 0.

These cases are taken from the following records: The Register of Births and Deaths, that of the Lagos Hospital, of the Ereko Dispensary, of the Government Prison, and from the Government daily sick list. To these is added for the past two years the record of the Massey Street Dispensary, which, being additional to the former, might have been expected to show a marked increase in the number of recorded cases, and I think we may say would certainly have done so instead of indicating a farther and continuous decrease, had there not been a strong counteracting agency at work (for it must also be remembered that the population of Lagos Town has been increasing during these years—the census of 1901 gave a population of 39,387, as compared with 32,508 in 1891 for natives, and 233 as against 143 for Europeans).

This counteracting agency, I venture to think, may be justly considered to be the prophylactic measures adopted by us during the time under review, namely, anti-mosquito measures and the administration of quinine, for I imagine the actual treatment of attacked persons was much the same in bygone times as it is nowadays in West Africa, so far as Europeans are concerned.

VI.—ANDREW DUNCAN, M.D., B.S.Lond., F.R.C.S., M.R.C.P.,
Physician, Seamen's Hospital Society; Lecturer on Tropical Medicine,
the London School of Tropical Medicine.

I PROPOSE to limit my remarks to the prevention of malaria amongst the troops in military expeditions, and more particularly to the mobile columns of the same. The preponderating effect in disablement that malaria causes in campaigns has ever been manifest. It will suffice to mention as examples the Ashanti expedition of 1896, in which one-half of the troops employed were placed *hors de combat* by African remittent fever; the Spanish-American war, in which out of 13,657 men in hospital at Cuba 2,014 were cases of malaria; whilst in our Burmese, China, Abyssinian, Looshai, Duffla, Malay, and Afghan wars, amongst others, malarial fevers either headed the list of admissions or took the second place. What, then, are the means whereby we may prevent this large disabling influence?

Before proceeding to the more especial means of prevention in accordance with the true etiology of malaria, I would first draw attention to the avoidance of the predisposing elements in matters connected with:

1. The selection of the men.
2. The age of the men.
3. Race.
4. The season for operations.
5. The clothing.
6. The period of marching.
7. The camping grounds.
8. The food.
9. The precautions at night.

First in the selection of the men. Here it must be borne in mind that previous attacks of malaria render the subjects of them more prone to future attacks. Hence the medical histories of a regiment must be closely scrutinized. For example, in the Duffla expedition of 1874, the 16th M.I. had for the eleven months previous to the outbreak of hostilities suffered greatly from "fever" in Assam; the 44th L.I. had for seven months before the expedition been stationed in the malarious plains

Among officials it was possible to secure a greater

also of Assam; in the campaign the 16th M.I. had 29.06 per cent. admissions for malarial fevers, the 44th Assam L.I. 18.40 per cent. for the same cause, rates far above those of the other regiments not presenting the malarious history which had admissions ranging from 3.60 per cent. to 7.35 per cent. only. Again, in the Afghan war my old regiment, the 14th S. Hs., one of the finest regiments in the Bengal Army, suffered greatly from malaria; previous to the campaign they had been for three years stationed in the malaria-stricken Peshawur Valley.

But not only does a malarious history predispose to malarial disease, but it also powerfully influences the onset of other diseases, especially amongst native soldiers—for example, pneumonia—it is also one of the common causes of cardiac dilatation. A regiment with a clean malarial history presents a great contrast to those cited above. I was some time in charge of the 23rd Pioneers in the Afghan campaign. They had come down from the hills of Simla in rude health, and no regiment during the war gave a better medical history. Hence we learn that regiments that have suffered markedly from malaria previously should not be chosen. The severer and more fatal forms of malaria never, as a rule, attack those previously immune from it.

Regarding age, it has been found that young soldiers and recent recruits suffer more than their older comrades. This was well shown in the Ashanti expedition of 1895-6.

Respecting race, it has been found that the negro race possesses the greatest immunity; in the War of the Secession in America, negro troops suffered the least from malaria, whilst a like experience was obtained in the Mexican war.

As regards season, meteorological influences speak with no uncertain voice. The end of the hot season in the tropics, when great alternations between the morning and evening temperatures occur, is very predisposing to malaria; again, the most dangerous time in the United Provinces in India is at the drying up of the rains. Hence this period of the year should be avoided if possible for military expeditions. A striking example of the above facts was shown in Ashanti in 1873. Here previous to the arrival of Sir Garnet Wolseley certain military operations had been carried on in the unhealthy season of the year. The Marines became so stricken with malaria that they had to be sent home, the mortality of the strength disembarked being 17.30 per cent. Sir Garnet operated in the succeeding healthy season, and the mortality for the strength was only 3.14 per cent.

Respecting clothing, flannel should be worn. Khaki cotton drill is not a good material to wear. At my recommendation in Afghanistan the men of the regiment were given a woollen jersey each, and the admission for malaria at once decreased. Cotton drills become damp and clammy in a humid climate. In the Perak expedition, for instance, it was a great source of fever. Flannel protects against the great alternations of heat and cold, of the day and night temperatures, inasmuch as by it a more equable temperature is insured.

As regards the period of the day for marching some authorities, such as Morache and Sanderson, advise not to march in the early morning. The late Professor Parker recommended doing so in the afternoon in hot malarious climates. I would, however, differ from this teaching on account of the danger of sunstroke. I never observed any ill effects from marching in the early hours in Afghanistan, whilst the men were far more knocked up whenever we did march in the afternoon. In Ashanti also it was found that although malaria was most powerful in the early morning, yet less harm occurred when marching in these hours than when proceeding for a length of time in the sun. The men should always have a meal of coffee and bread previous to setting out, and should also smoke in moderation, as Sanderson found by experience the good effect of tobacco in warding off attacks of malaria when marching in the early morning. At the end of the march, sitting down in wet clothes should not be permitted, and arrangements should be made whereby the men always have their great-coats handy. In the Afghan war the 14th Sikhs were without their great-coats, and for nearly three weeks slept on the ground with only their ordinary uniform. Malarial fever was exceedingly prevalent amongst them. The cholera belt should always be worn.

As regards the camping grounds, the exigencies of the military operations must of course always be of chief weight. But as showing the influence of healthy camps over the reverse, it may be mentioned that during the Zulu war the second division enjoyed a high state of health, being encamped and operating on high bracing uplands. The first division, on the contrary, was on the hot swampy coast-line,

and suffered much from fever. The American War of the Secession gave us a striking example of this in the force occupying the Atlantic coast: where there were huge alluvial tracts the intermittent fever-rate was 194.03 per 1,000, as against 121.77 for the Pacific region, where only a narrow strip of land intervened between the sea and the mountains. Again, especial precautions should be undertaken where the necessities of warfare entail much disturbance of the soil, as in making fortified places. This was shown in the large amount of malarial fever that attacked our men in the Zulu war during the erection of Fort Chelmsford. In the French army, again, at Madagascar, the men employed in road-making died like flies.

Turning now to the food of the men, the ration must be sufficient, and it should not be a monotonous one. Livingstone in Africa found plenty of wholesome food protected his men more than quinine. Tea and coffee should be as far as possible habitually drunk, both on the march and in camp. Koch recommends that all water be boiled in the prevention of malaria. In the Niger campaign the only white officer who remained free from the disease boiled his water. The late Sir Henry Stanley used to emphasize the importance of sterilizing water in combating malaria. In the Terai the Forest Department, by constructing deep masonry wells, have enabled their employes to remain permanently at work at the station at which they had formerly to be relieved periodically every fortnight on account of the deadly malaria.

With regard to the precautions to be taken at night: where arrangements can be made, the men should not sleep on the ground. In the Spanish-American war it was reported in the *New York Medical Journal* that "nearly all those who slept on the ground after the battle of July 1st to 3rd contracted some form of malarial fever, the worst cases being in those who had to sleep on wet ground with only their army blankets for bedding. Every one of a number of men who slept on wet ground at a certain spot near 'Bloody Bend' died later of pernicious malaria." Mr. Sanderson, whose life was spent in the jungles of Chittagong after elephants, attributes the fact that he was remarkably free from malaria to two facts: (1) that he always slept off the ground on a bedstead, and (2) that he always slept within mosquito nets. This last fact brings us to the methods by which we may attack the *fons et origo mali*. Malarial fever may be prevented in three ways:

1. By malarial protection against the infected mosquitos.
2. By destruction of the infected mosquitos.
3. By prophylactic drugs.

With regard first to mechanical protection: any one who has been on a campaign must recognize the fact that for an army on the march protection by mosquito nets is out of the question. What chance would there be for the net remaining intact during the daily packing and unpacking of the impedimenta. Again, in these days, when one's luggage is cut down to a minimum, so that a tent and portable bedstead are even out of the question, how could a mosquito net be fixed up? The officers of the force might, perhaps, arrange for such, but does any one imagine that Tommy Atkins would take care of his net? The destruction of infected mosquitos is also out of the question on the line of march. There remains, therefore, the protection to be obtained by the administration of prophylactic drugs. There is a vast amount of evidence from all parts of the world in favour of this method. There is also a certain amount of evidence showing this method to have failed. Thus it is curious that both in the Ashanti war of 1873 and in the last one, the principal medical officers did not find the practice of any value. The same occurred in the Malay war of 1875-6. In the Gambia expedition the men taking quinine had as much fever as those not taking it. But, on the other hand, there is overwhelming testimony in its favour. Time will not permit me to give all the instances in which it has been found to be beneficial to give prophylactics. What prophylactic should be used? During the latter period of my service in India I made comparative trials of fifteen different antimalarial drugs. The experiments were comparable, as they were all made on one race of men, namely, Goorkhas, and the men were all in the same station. The result showed that quinine was the drug *par excellence*. Next to quinine, powdered pine bark was most successful. Narcotine, recommended by the late Sir William Roberts, was disappointing. Arsenic I found to have no antimalarial action. It has, however, been found effectual by other observers such as Crudeli and Ricchi in Italy, Downie in India, and Leslie in Africa. Sulphur and hyposulphite of soda, recommended by Dr. Schmidt, were tried in the Mauritius epidemic of 1866-7, but found to be of no use.

With respect to the dose of quinine, this was given by me in three grains every morning.

VII.—KENNETH McLEOD, M.D. Edin. : Colonel I.M.S. (Retired),

Professor of Clinical and Military Medicine, Army Medical Staff College. COLONEL McLEOD remarked that there were several points in the papers just read which deserved comment. The conditions affecting the life of the mosquito were very properly mentioned as having an important bearing on prophylaxis. The food of larvae was not referred to, but this seemed a very essential condition. The conveyance of adult mosquitos by railways and other carriages, ships, etc., was not alluded to, although that was no doubt a possible means of dissemination. The relations of mosquitos to trees and plants was not dealt with by Dr. Stephens, but this point seemed also of some consequence. The use of the broom in sweeping away insects was referred to in one of the papers read. There could be no doubt that the clearing away of dust in dark corners where mosquitos lodged during the day in huts and houses ought to be scrupulously practised. In regard to the Mian Mir experiment a doubt arose as to the propriety of the selection. The conditions were evidently most complex and difficult. The influence of irrigation in the prevalence of malaria had long been known. The abolition of irrigation as a means of preventing malaria seemed hardly feasible or commendable. The alternative was between fever and famine but perhaps, by good drainage and other measures, this alternative need not necessarily arise.

VIII.—Major G. H. FINK, I.M.S. (Retired).

MAJOR FINK said: There are many points in Dr. Stephens's paper which place us in a very difficult position as to the prime origin of malaria while considering Ross's investigations with regard to the particular species of *Anopheles* in which he (Ross) had found the malarial parasite. The speaker previous had referred to the food of mosquitos, and I feel that the difference in the opinion as to the particular *Anopheles* in which the parasite of malaria is to be seen may possibly be due to the food, as well as the soil, as conditions favouring the life of the parasite or otherwise. An *Anopheles* mosquito which is harmful as regards malarial transmission in a clayey, moist soil and atmosphere, might be harmless in a sandy, dry soil and atmosphere. With regard to this particular point of finding certain conditions in cells, I would like to state that Professor Heitzman of Vienna had in his investigations found a nucleus always present in "unicellular organisms" which he had cultivated in Vienna, but the same organism presented no nucleus in New York. This would, perhaps, throw some light upon the subject. Plehn's "primitive bodies," or karyochromatophile granules, mentioned in Manson's book, have yet to be thoroughly investigated, since it is acknowledged that very little was known about them, except that they are to be seen in the blood of Europeans on the West Coast of Africa soon after their arrival there, but attended with anaemia, and this before malaria has pronounced itself in the individual. It might be considered as a "latent phase" of malarial fever parasites, but an organism on the borderland between the vegetable and the animal, which presented both the male and the female element, which had to pass through the usual cycle before its exact position could be recognized in the body of the mosquito, and before malarial fever was transmitted through the bite of that insect to man. With regard to prophylaxis of malaria many substances are mentioned in books; but peppermint oil is not given as a culicicide or culicifuge. I tried it in Sibsagar district, Assam, in my own house with good effect, and found it superior to petroleum. I also tried it as an emulsion with castor oil and liquor potassae, which I sprinkled over the stagnant water of a quadrangular moat around the station; the mosquitos on the side of the moat where it had been tried were lessened in numbers, and the fever cases on that side diminished greatly. I felt that such an experience was worth recording, and have only come forward with my remarks since I believed that we can not learn too much about malaria and its prevention in every country from the lips of all interested in the subject.

IX.—P. O. MALABRE, M.D.

DR. MALABRE inquired as to the reputed destructive power of dragon flies on mosquitos. It had been said that the dragon

fly preyed upon mosquitos, and it would seem a possible method of destroying mosquitos and lessening malaria to introduce the natural enemies of the mosquito into regions where they prevailed and where malaria was rife. In Jamaica smoke from various substances was also an efficient means of keeping down the number of mosquitos.

X.—Captain C. E. WILLIAMS, I.M.S.

CAPTAIN WILLIAMS said: Dragon flies in all parts of the world prey on mosquitos, which they usually seize when on the wing. I have seen it reported that in some parts of the tropics residents keep dragon flies in their rooms, with a view to reducing the number of mosquitos, but I cannot remember the reference just now. In Hudson's *Naturalist in La Plata* it is related that whenever dragon flies were present the mosquitos, which had previously been a pest to residents, at once disappeared, and only reappeared when the dragon flies left the vicinity. The mosquitos appeared to realize the danger from the dragon flies, and to hide themselves until it had passed. I have lately raised dragon flies from the larvae, in captivity, and fed them on living mosquitos, which they seized when on the wing and fed upon. So far as I know, the dragon fly does not feed after dusk in the tropics; my personal observations relate to India and Burmah. The minute nymphs of various species of mantis feed readily on mosquitos, and possibly other insects of the same size. I have raised many hundreds from the egg, and fed them for the first six weeks entirely on mosquitos, upon which they thrived most satisfactorily.

XI.—FRITZ I. A. BERINGER, M.R.C.S., L.R.C.P., Colonial Service, Gold Coast.

DR. BERINGER said that in regard to the use of quinine as a prophylactic he was in Hong Kong doing duty with troops from 1900 to 1902. Quinine sulphate, 5 gr. in solution, was given to European troops every morning at quinine parades, beginning in 1900, at the time when the malarial season usually commenced; later 10 gr. per day on three consecutive days per week were administered. During this year a greater percentage of cases than during the previous five years was noted. In one month 14 per cent. of all European troops were in hospital for malarial fever. At the same time it must be stated that this was a bad malaria year amongst the civil population. The general opinion was that the cases of fever were perhaps a little less severe. The result undoubtedly showed that quinine in this instance had no effect on the number of cases of malaria. In West Africa also he had seen several cases of malaria in those who had taken quinine regularly. The diagnosis rested on clinical evidence in the great majority of Hong Kong cases, but many were examined microscopically and the malarial parasites found.

XII.—Sir PATRICK MANSON, K.C.M.G., F.R.S., Medical Adviser, Colonial Office.

SIR PATRICK MANSON asked if the diagnosis of malaria in the case of the Hong Kong garrison in whom quinine failed to exercise its usual prophylactic influence was correct, how was it arrived at? Possibly the fever from which the garrison suffered was not malarial. Hitherto many places in the tropical world in which the physical conditions seemed to be suitable for malaria had remained immune. That such places might become infected subsequently was evidenced by the recent history of Mauritius. Might not many of the islands of the Pacific—Fiji, Tahiti, Samoa—become similarly infected? Would it not be advisable to be prepared beforehand against such a contingency?

XIII.—DANIEL ELIE ANDERSON, M.D. Paris, M.B. Lond.

DR. ANDERSON said: Whilst on a visit at Tamatave, Diégo Suarez, and Nossi Bé, in Madagascar, in 1889, I noticed and was told that the natives cooked their food in the one-roomed hut, and the only outlet for the smoke was a hole in the apex of the cone-like roof; thus the room was constantly filled with smoke and became free from mosquitos. It is a known fact that in Mauritius and elsewhere in the tropics, and also along the Riviera, substances producing a large quantity of smoke are burned in the bedrooms to drive away the mosquitos. The smoke does not kill the mosquitos, but numbs them; for hours they remain quiet and will not fly about. I have over and over again tried this method in Paris in summer to obtain a few hours' sleep. Everybody living in the Riviera (in Mentone, Nice, etc.) will corroborate what I state. I beg to

corroborate Sir Patrick Manson's statement that the malarial *Anopheles* can be introduced into places where it never existed before, as in Mauritius, for instance, where in 1867 it was brought over probably from India or Madagascar. Malarial fever now is endemic in that island. Why the malarial *Anopheles* had not made its introduction before, seeing that immigration of coolies from India had existed for a score of years anteriorly, and there had been constant communication with Madagascar, whence bullocks for the market are brought—why, I say, the malarial *Anopheles* had not propagated itself in larger numbers before 1867 in Mauritius is until to-day a subject of discussion (*vide* my Notes on Malarial Fever in Mauritius, August, 1890). There is no doubt that before 1867 sporadic cases of malarial fever were known in Mauritius. During my visit to the island of Mauritius in 1899 I came across several Indians suffering from filariasis, and in most of them I traced the cause to eating the watercress growing in ponds of stagnant water. Evidently the eggs of the filaria had been deposited on the under surface of the leaves of the watercress. In Madagascar (Nossi Bé), which I also visited, where filariasis is common, the remedy is aq. chloroformi (B.P.), $\frac{3j}$, two or three times in the two hours, until patients fall asleep; then the symptoms begin to disappear.

XIV.—W. J. SIMPSON, M.D. Aberd., F.R.C.P.,

Professor of Hygiene, King's College, London; Lecturer in Tropical Hygiene, London School of Tropical Medicine.

PROFESSOR SIMPSON remarked that the only observation he had to make was that he regretted that the experiment against malaria had been made at Mian Mir. It was not under conditions that were likely to be successful. Only a small portion of the cantonment was dealt with instead of the whole, which after all only covered an area of four miles. If a proper and efficient organization had been used over the whole cantonment probably the results would have been very different. The Government of India were well known to expect their medical officers to make bricks without straw. Other experiments were proposed to be made in the central provinces. The Sanitary Commissioner proposed that the civil surgeons of five municipalities should carry out a campaign against the mosquitos. The civil surgeons were to search out for the localities in which the *Anopheles* reproduced themselves, to take measures for the destruction of the mosquitos, and to note the results in their annual reports. Now, at the Indian Medical Congress held in Calcutta in 1894 he (Dr. Simpson) had shown that it was perfectly impossible for a civil surgeon to carry out any sanitary duties, his work in other directions being so overwhelmingly great. He had to visit the gaol and attend to the prisoners, also the lunatic asylum and the hospital, to superintend vaccination, besides performing many other governmental duties, as well as those of medical attendant to the civil servants and others of his district. No civil surgeon had any time to carry out any malarial campaign, and it was hoped that no such experiment would be attempted, for it would only lead to confusion and to failure. Nothing of this kind could be effected without a proper sanitary organization, which India was without at the present moment, but which he hoped, for the sake of the welfare of the Indian population, would be soon created.

XV.—ALEXANDER CROMBIE, M.D.,

President of the Section.

THE PRESIDENT said: We have now before us two great object lessons. Not to speak of the brilliant results which are reported from Havana, we have the success which followed the measures taken at Ismailia, which have made it a mosquito free and malaria-free sanatorium, and we have the equally signal failure of those carried out at Mian Mir, and which have failed to mitigate in the smallest degree the incidence of malaria in that cantonment, and it behoves us to ask ourselves why the same or similar measures for the destruction of mosquitos were successful at one place and failed at the other. The answer is not difficult to find. At Ismailia the means adopted were adequate, were carried out in a whole-hearted way, were applied to the whole and every part of the town, and included the adjoining marshes. Not only was the usual anti-mosquito campaign carried out in the town, but the neighbouring marshes were drained, and their stagnant water converted into running streams. The whole area was rendered unfavourable for the propagation of mosquitos. At Mian Mir, on the other hand, only a small fraction of the area was brought into the field of operations; only one corner of the

cantonment was treated, and the remaining three-fourths of the area was left untouched, with its network of irrigation canals, its irrigated crops up to the very doors of the bungalows, a very paradise for the breeding of mosquitos, its pools of standing water after each shower, and no adequate provision was made to prevent the influx of *Anopheles* from outside, of larvae in the water of the canals, and of adults from the irrigated fields outside and inside the limits of the cantonment. An experiment conducted on that scale was doomed to failure. Would it have succeeded at Ismailia if the measures had been restricted to one quarter of the town, and nothing been done to prevent the immigration of *Anopheles* from the adjoining marshes? I think we may say that if the measures advocated by Ross at Ismailia had been carried out in the same partial and half-hearted way the failure there would have been just as complete as it has been at Mian Mir. Another thing has come out of the discussion, and that is the astounding fact that in the year of grace 1904 the cultivation of crops for which irrigation is essential is permitted within the limits of a cantonment for British troops. The danger to health of the cultivation of rice and other crops requiring irrigation and standing water has been recognized from all time. I was lately reading a book (*Epidemiologia Española*), published at Madrid in 1802, in which the following passage occurs: "The cultivation of this plant (rice) has often been considered contrary to public health. From the year 1324, little more than a century after the conquest of the Kingdom of Valencia, we meet with the prohibition of the growth of rice in the neighbourhood of the capital, and in that of some of the surrounding towns. This prohibition was repeated by the same sovereign, Don Pedro the Second, in 1386, by King Don Martin in 1403, and by Don Alonso in 1483." In the memoirs of the Royal Academy of Medicine of Madrid, printed in 1797, are some excellent observations "on the cultivation of rice in the Kingdom of Valencia, and its influence on public health," by Don Antonio Josef Cabanilles. He attributes the unhealthiness of regions where rice is grown to the multitudes of insects which are produced in these marshy situations, leaving there their excrement and dead bodies, and points out that the colour of men dwelling in these regions is changed, and tertians manifest themselves, which with time augment in strength and malignancy. He insists on the prohibition of the cultivation of rice and the drainage of the fields where it is grown as the remedial measures indicated. And yet we have, six hundred years after the royal edicts I have alluded to, the all but incredible fact that the cultivation of rice and other irrigated crops is permitted inside a British cantonment. I do not think that Colonel McLeod was quite fair to Captain Sewell in this respect. That officer does not demand the abolition of irrigation throughout the length and breadth of India, but he advocates its abolition only within the limits of Mian Mir, and I think we must all agree with him that there can be no diminution of the prevalence of malaria, or of the inefficiency which it causes among the troops stationed there till it is abolished. One of two things must be done; either the irrigation system which now exists must be swept away, or the cantonment of Mian Mir must be removed to a more healthy situation. The result of the anti-malarial experiment carried out in that cantonment shows conclusively that half measures are futile.



